Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

That is to say, for this S_2, $A(S_2)$ does not imply $M(S_2)$. The formal implication falls to the ground.

Further on Mr. Lenzen says that a presumption is created that independence applies to propositions as well as to postulates. This presumption is probably an error, for any method of deduction involves the consideration of the particular propositions as special cases of propositional functions. If deductive relations could be found between propositions, these relations would also hold between the corresponding postulates, or else the propositions would not be typical cases. Contrary to Mr. Lenzen's assertion, it is possible sometimes to find deductive relations between particular propositions which correspond to independent postulates, if non-typical cases are chosen. The writer has found several but will choose one for the sake of illustration.

Consider the postulates

$$\text{(A)} \qquad a p b = b p a$$
$$\text{(B)} \quad a p (b p c) = (a p b) p c$$

These are independent, for if $a p b$ means $(a+b)/2$, A is true while B is not; if $a p b$ means a, B is true while A is not; if $a p b = a b$, both are true.

Consider a particular case of B, namely,

$$a p (b p a) = (a p b) p a.$$

This can be deduced from (A) for

$$\begin{aligned} a p (b p a) &= a p (a p b) \\ &= (a p b) p a, \end{aligned}$$

using A twice.

P. J. Daniell.

Rice Institute, Houston, Texas.

CAUSE AND EFFECT.

I.

The latest book by Mr. Bertrand Russell (*Mysticism and Logic, and Other Essays*, London and New York: Longmans, Green and Co., 1918; pp. viii, 234; price 7s. 6d. net) consists of reprints of ten previously (1901-1915) published papers which form a more or less complete whole and to which are added one or two notes written in 1917. The first essay, on "Mysticism and Logic," appeared in

The Hibbert Journal for July, 1914, and contains an attempt at a characterization of the scientific and the mystical impulses. Much of this essay is familiar to most of us from Mr. Russell's book on *Our Knowledge of the External World* (Chicago and London, 1914, cf. especially pp. 1-30, 45-47, 63-64, 95). "The Place of Science in a Liberal Education" appeared in *The New Statesman* for 1913 and is concerned especially with the intrinsic value of a scientific habit of mind in forming our outlook on the world. "A Free Man's Worship" and "The Study of Mathematics" are reprinted from Mr. Russell's *Philosophical Essays* (London and New York, 1910), now out of print, where they had been reproduced from magazine articles of 1903 and 1907. The first of these two essays is supplemented by a note in the Preface (p. v) drawing attention to an alteration in Mr. Russell's ethical views. The essay on "Mathematics and the Metaphysicians" appeared in *The International Monthly* for 1901 under the title "Recent Work in the Philosophy of Mathematics" (cf. *The Monist,* Vol. XXII, 1912, pp. 149-158). It is interesting to read that the tone of this essay is partly explained by the fact that the editor begged Mr. Russell to make the article "as romantic as possible" (p. vi). This reprint is very useful since the original seems difficult to obtain, and the amusing and slightly frivolous remarks about mathematics have been so often taken literally and thus misunderstood by superficial and serious people. The "Herbert Spencer Lecture" of 1914 "On Scientific Method in Philosophy" was first published in that year, and here again there is not much that will be new to those who have read Mr. Russell's larger book mentioned above. "The Ultimate Constituents of Matter" was published in *The Monist* for July, 1915; "The Relation of Sense-Data to Physics" was published in *Scientia* for 1914; and "Knowledge by Acquaintance and Knowledge by Description" was published in the *Proceedings of the Aristotelian Society* for 1910-11. It will be found that most of the views in these essays are either repetitions or developments of the views given in the above book or in Mr. Russell's *Problems of Philosophy,* first published in 1912 in London and New York.

The only remaining essay is "On the Notion of Cause," which was published in the *Proceedings of the Aristotelian Society* for 1912-13, and also, though this is not stated by Mr. Russell, in *Scientia.* This essay contains some things which do not appear in either of the books just mentioned, but most of those that do not so appear go back to Mr. Russell's out-of-print *Principles of Mathematics* of 1903. It seems to me that this essay suffers from the fact that the view that the concept of *cause* can be replaced by the mathematical concept of *function* has been almost a commonplace of science since Mach wrote,—say between 1866 and 1883,—and that Mach's name is not mentioned. The essay is thus summed up (pp. 207-208): "We found first that the law of causality, as usually stated by philosophers, is false, and is not employed in science. We then considered the nature of scientific laws, and found that, instead of stating that one event A is always followed by another event B, they stated functional relations between certain events at certain times, which we called 'determinants,' and other events at earlier or later times or at the same time. We were unable to find any *a priori* category: the existence of scientific laws appeared as a purely empirical fact, not necessarily universal, except in a trivial and scientifically useless form. We found that a system with one set of determinants may very likely have other sets of a quite different kind; that, for example, a mechanically determined system may be teleologically or volitionally determined. Finally we considered the problem of free will: here we found that the reasons for supposing volitions to be determined are strong but not conclusive, and we decided that even if volitions are mechanically determined, that is no reason for denying freedom in the sense revealed by introspection, or for supposing that mechanical events are not determined by volitions. The problem of free will *versus* determinism is therefore, if we were right, mainly illusory, but in part not yet capable of being decisively solved."

Just as in the *Principles,* Mr. Russell considers the case of a swarm of particles in abstract dynamics, where the only thing which, it seems, might correspond to what is called "causality" is the fact that, since the whole path of the swarm is determined by a system of certain ordinary differential equations of the second order, all the arbitrary constants are fixed if we know the configurations of the swarm at any two given times, and thus the configuration at any other instant whatever is uniquely determined by the above differential equations and the above two fixed configurations. Mr. Russell does not state this well-known fact clearly, and thus the uninitiated are left dumb with admiration at what seems mysterious penetration of a very brilliant man. If Mr. Russell had put the matter frankly, he would certainly have committed what would doubtless seem to him a double crime of stating a technical platitude, but still it does not seem that it would have escaped him or others that, in this discussion, the question as to what meaning can be given to "causality" is implicitly limited to swarms whose paths are defined by ordinary differential equations of the second order, so that there are unanalyzed assumptions as to the nature of the functions which give the dependences of the coordinates on the time which are fundamental, and that the grounds for generalization to other physical "swarms" ought to be given.

The logical analysis (pp. 180-192) of what philosophers, according to the definitions they have given, have professed to understand by the word "cause" and allied words is often very amusing. Still this analysis is based on principles which are quite well known to modern logicians; for example, a discussion (such as that on p. 187) of such statements as that the effect follows the cause "immediately" can, of course, quite easily reduce the statement to nonsense by the remark that there are no such things as "consecutive points" in space or time. Then we come to the really important part of the article: "In spite of these difficulties, it must, of course, be admitted that many fairly dependable regularities of sequence occur in daily life. It is these regularities that have suggested the supposed law of causality.... I do not deny that the observation of such regularities, even when they are not without exceptions, is useful in the infancy of a science.... What I deny is that science assumes the existence of invariable uniformities of sequence of this kind, or that it aims at discovering them.... In short, every advance in a science takes us farther away from the crude uniformities which are first observed, into greater differentiation of antecedent and consequent, and into a continually wider circle of antecedents recognized as relevant" (pp. 187-188). Again, "such laws of probable sequence, though useful in daily life and in the infancy of a science, tend to be displaced by quite different laws as soon as a science is successful" (p. 194).[1] Thus, in the motions considered in an advanced science such as gravitational astronomy, "there is nothing that can be called a cause and nothing that can be called an effect; there is merely a formula. Certain differential equations can be found, which hold at every instant for every particle of the system, and which, given the configuration and velocities at one instant, or the configurations at two instants, render the configuration at any other earlier or later instant theoretically calculable. That is to say, the configuration at any instant is a function of that instant and the configurations at two [fixed instants]. This statement holds throughout physics and not only in the special case of gravitation" (p. 194). This, then, brings us back to the point of view of the *Principles.*

---

[1] Cf. *Ext. World,* p. 220.

Then Mr. Russell proceeds with the replacement of the old "law of causality" by the idea of a mathematical "function": "No doubt the reason why the old 'law of causality' has so long continued to pervade the books of philosophers is simply that the idea of a function is unfamiliar to most of them, and therefore they seek an unduly simplified statement. There is no question of repetitions of the 'same' cause producing the 'same' effect; it is not in any sameness of causes and effects that the constancy of scientific law consists, but in sameness of relations. And even 'sameness of relations' is too simple a phrase; 'sameness of differential equations' is the only correct phrase" (pp. 194-195).[2] "If the 'law of causality' is to be something actually discoverable in the practice of science, the above proposition has a better right to the name than any 'law of causality' to be found in the books of philosophers.... No one can pretend that the above principle is *a priori* or self-evident or a 'necessity of thought.' Nor is it, in any sense, a premise of science: it is an empirical generalization from a number of laws which are themselves empirical generalizations" (p. 195).

The old "law of causality" is not assumed by science, but "something which we may call the 'uniformity of nature' is assumed, or rather is accepted on inductive grounds. The uniformity of nature does not assert the trivial principle 'same cause, same effect,' but the principle of the permanence of laws. That is to say, when a law exhibiting, e. g., an acceleration as a function of the configuration has been found to hold throughout the observable past, it is expected that it will continue to hold in the future, or that, if it does not itself hold, there is some other law, agreeing with the supposed law as regards the past, which will hold for the future. The ground of this principle is simply the inductive ground that it has been found to be true in very many instances; hence the principle cannot be considered certain, but only probable to a degree which cannot be accurately estimated" (p. 196; cf. p. 192).

---

[2] Cf. what seems to be a contradictory statement in *Ext. World*, p. 214.

We will now give a short account of the not dissimilar views previously published by Mach and Karl Pearson.

In papers published in 1865 and 1866, Mach (cf. *Conservation of Energy*, Chicago and London, 1911, pp. 89-90; cf. p. 95; this book is a translation of an important work published by Mach in 1872) maintained that in physics "space" and "time" are merely shorthand symbols used in the representation of the dependence of phenomena on one another. In the work of 1872 just referred to Mach replaced the concept of *cause* by the concept of *function* (cf. *Mechanics,* 3d ed., Chicago and London, 1907, p. 555, and *C. of E.,* p. 9), and also shortly expressed his fundamental standpoint in psychology, physics, and psychophysics (*C. of E.,* pp. 91, 95) as sets of inquiries into the connections among themselves and with each other of (1) our presentations, and (2) our sensations.

According to Mach, the "law of causality" is "the presupposition of the mutual dependence of phenomena" (*C. of E.,* p. 61; cf. p. 102). "The business of physical science is the reconstruction of facts in thought.... The rules which we form for these reconstructions are the laws of nature. In the conviction that such rules are possible lies the law of causality. The law of causality simply asserts that the phenomena of nature are dependent on one another. The special emphasis put on space and time in the expression of the law of causality is unnecessary, since the relations of space and time themselves implicitly express that phenomena are dependent on one another" (*M.*, p. 502). Indeed, "we have grown used to considering natural phenomena as dependent upon one another" (*C. of E.,* p. 59); and, since "temporal" and "spatial" determinations are merely, as indicated above, determinations of phenomena by means of other phenomena, we can eliminate the mention of time and space in Fechner's formulation of the law of causality: "Everywhere and at all times, if the same circumstances occur again, the same consequence occurs again; if not, not" (*ibid.,* pp. 60-61; cf. p. 98). Thus the law of causality is the supposition that between the phenomena $\alpha, \beta, \gamma, \ldots, \omega$ certain equations subsist, the number and form of which are to be found empirically (*ibid.,* pp. 61-62); but we can never discover anything which one would try to express by the phrase "the behavior of the totality of phenomena" (*ibid.,* pp. 62-63). "Let us call the totality of phenomena on which a phenomenon α can be considered as dependent *the cause* [of α]," then α is determined *uniquely* by the cause (*ibid.,* pp. 63-64; cf. *M.*,

p. 502). The principle of "sufficient reason," which was used in mechanics by Archimedes, Poinsot, and others, is, in a paper published in 1868, "only another form of the law of causality" (*ibid.*, p. 81). In 1872, it is the "inverse of" (*ibid.*, p. 65), or is "not essentially different from" (*ibid.*, p. 66) that law (*ibid.*, pp. 65-69; cf. *M.*, p. 502), and is, like that law, barren in default of positive experiences. The principle of excluded perpetual motion is another form of the law of causality: "If a group of phenomena is to become a source of continual work; this means that it shall become a source of continual variation of another group of phenomena. For, by means of the general connection of nature, all phenomena are also connected with mechanical phenomena, and therefore with the performance of work. Every source of continual variation of phenomena is a source of work, and inversely" (*C. of E.*, p. 69). Then some simple consequences of the phenomena α, β, $\gamma, \ldots$ being one-valued functions of $x, y, z, \ldots$, were deduced, and the facts emphasized (1) that these theorems do not apply merely to mechanics, and (2) the barrenness of the theorems without experiences (*ibid.*, pp. 69-71; cf. also *M.*, pp. 502-504).

Thus, "the theorem of excluded perpetual motion is merely a special form of the law of causality, which law results immediately from the supposition of the dependence of phenomena on one another—a supposition which precedes every scientific investigation; and which is quite unconnected with the mechanical view of nature, but is consistent with any view if only it retains a strict rule by law" (*C. of E.*, pp. 73-74). This theorem, indeed, is reducible to the purely logical truth that, if $\lambda, \mu, \nu, \ldots$ are one-valued functions of $\alpha, \beta, \gamma, \ldots$, and $\alpha, \beta, \gamma, \ldots$ pass to values $\alpha', \beta', \gamma', \ldots$, so that $\lambda, \mu, \nu, \ldots$ pass into $\lambda', \mu', \nu', \ldots$, then, if the set $\alpha', \beta', \gamma', \ldots$ be brought back to $\alpha, \beta, \gamma, \ldots$, the set $\lambda', \mu', \nu', \ldots$ will return to $\lambda, \mu, \nu, \ldots$ (*M.*, p. 503).

* * *

The views of Karl Pearson are to be found in his book entitled *The Grammar of Science* (2d ed., London, 1900), and seem to have been developed independently of those of Mach. Pearson vigorously protested (cf. pp. 78, 81, 112) against the use of the word "law" when speaking, for example, of gravitation, and suggested the substitution of the word "formula." This is, of course, practically the same thing as the replacement of the notion of *cause* by the conception of *function*. Pearson's work is perhaps better known than that of Mach, but, from the point of view of theory of knowledge, it is very inferior to Mach's work. For instance, the treatment of causality and probability (pp. 128-129, 130) may be mentioned.

* * *

In what follows we will indicate the changes introduced, from 1912 onward, in Mr. Russell's views of the nature of causality,—its dependence on the "principle of induction" and so on. Then we will state some objections, which seem to me to be quite conclusive, to these later views; and finally we will give a sketch of a theory of causality as an *a priori* principle together with a suggested definition of "probability," in so far as it is not merely a way of admitting ignorance as to the validity of certain deductions from completely determined premises,—as when, for example, we say "it is probable that the proposition p implies the proposition q."

According to Mr. Russell (*Principles of Mathematics,* Cambridge, 1903, p. 478), "Causality, generally, is the principle in virtue of which, from a sufficient number of events at a sufficient number of moments, one or more events at one or more known moments can be inferred." This principle, which seems to be the same as the one which, as we have seen, Mach accepted as *a priori,* was founded, in Mr. Russell's *Problems of Philosophy* (first published in 1912, pp. 93-107), on the "principle of induction." Indeed, if we eliminate the reference to time in the manner indicated by Mach, there appears to be complete identity between the above definition of Mr. Russell and the previous definition of Mach. It is, however, convenient to preserve the reference to time when we are considering the abstract science of mathematical physics. We consider an aggregate (which we may call the "space-time" aggregate) of real numbers in four dimensions which forms a numerical image (x, y, z, t) of our physical space and our physical time. This aggregate forms what we may call "absolute" space and time, and is presumably what Newton meant to indicate by certain well-known words of his. With this apparatus of space and time, the positions and motions of physical particles and bodies are represented by functions defined in the aggregate. The aggregate must be numerical if the motions are to be described by differential equations, for the same reasons that, as Mr. Russell pointed out in his *Principles,* the concepts of *differential coefficient* and *integral* imply *numerical* aggregates and not merely any ordered aggregates.

Since an "event" in abstract dynamics is a configuration (*Principles of Mathematics,* p. 486), the principle of induction is simply a problem of extrapolation; if we consider various particular values of an unknown function to be given and which represent various events,—such as the position of the center of a moving sphere as a function of time,—we have to seek a principle in virtue of which we can conclude the values of such a function at other times. It does not affect the nature of the principle if these other times are in the future or the past (cf. *Problems,* p. 101; *Ext. World,* p. 224). Of course, if the functions are quite general in their nature, no such inference can be made; so that if such inferences are to be possible, the functions in question must be of such a nature as to allow inferences from values given to values not given. Now, if a function of t is continuous and the values of the function for a certain infinite aggregate of values of t are known, we can conclude the values of the function at every point which is a limiting point of the t-aggregate, a part of the space-time aggregate just referred to. If the function t is differentiable, the same thing can be done; but if it is analytic, if we know its values for any period of time, however short, the mere knowledge that it is analytic enables us to conclude its value for any other value of t whatever. If, then, we have any reason to suppose that the functions which we assume to lie, as a subject for investigation, at the bottom of natural phenomena are of any special nature, this nature may enable us to give some definite information as to the form in which we can apply induction.

There is a doubtful point about all this: we assumed, in agreement with Mach, that the mathematical physics contemplated depends on functions which are at least theoretically known for all the values of the time; but it might seem that we can only know that nature is governed by law at all times by an application of the inductive principle (cf. *Problems,* pp. 98-103, 107). This objection was urged against me in a conversation of 1912 or 1913 with Mr. Russell, when I tried to explain the point of view of investigating the foundations of mathematical physics by determining the natures of the various functions used (cf. my notes in Mach's *C. of E.,* pp. 99-101). But it seems to me that there are two reasons against regarding induction as more fundamental than causality.

1. In the first place, mathematicians have become accustomed, at any rate since the times of Dirichlet, to regard the word "func-

tion" as meaning a correspondence between two aggregates (variables) even when the correspondence cannot be expressed by any known law. Thus, when we say that, for example, x is a function of t, we do not imply that, when t is fixed in value, the corresponding value of x can be calculated by a formula which expresses the law of correspondence: indeed, it can be established by simple arguments of which forms were used by Cantor as early as 1873, myself in 1903 and later, and Mr. Russell in *The Monist* for January, 1914, that there are functions in the general sense which cannot be represented as limits of infinite series of continuous functions. The importance of such reflections in this connection is that, when we say that x is a function of t, we do not imply that there is a formula correlating x and t; and that consequently we do not assume that x "depends" on t in the sense in which we would say that x "is given in terms of" t by a formula or "law of nature." It might be urged that such a function would be incapable of definition, but this would be a mistake, as is clearly shown by Cantor's method (1892) of defining uniquely a one-valued function which must necessarily be omitted from any one-one correlation of the arguments of this function with the class of one-valued functions possible for the same arguments, or by a simple application of my recent proof that any aggregate whatever can be well-ordered. It seems that Mr. Russell has been misled by his refusal to give ear to the teaching of the history of mathematics, and has paid the penalty of this refusal by repeating unconsciously the reasons which led D'Alembert, in the latter half of the eighteenth century, to maintain that the "arbitrary" functions which appear in solutions of certain partial differential equations obey the "analytic" law of being determined for the whole range of their arguments by the values for a small range of these arguments. In fact, in his formulation of the law of universal causation, Mr. Russell proceeds as follows (*Ext. World*, p. 221; cf. p. 219): "There are such invariable relations between different events at the same or different times that, given the state of the whole universe throughout any finite time, however short, every previous and subsequent event can theoretically be determined as a function of the given events during that time." That there is here an assumption that such relations are what Weierstrass understood by the words "analytic functions" can be proved quite simply by showing that we cannot,—excluding for obvious reasons the "monogenic" functions of Borel,—reduce the states at all times from the states during some interval of time when the function is not analytic. Suppose the state were known to be constant throughout the finite space of time from *a* to *b*, including the ends: if the function were analytic, we could conclude that the state was constant throughout all time; but this need by no means be the case if the function were merely continuous or, say, merely differentiable.

2. I now come to the objection to founding causality, through the "principle of induction," on the notion of probability. The world of mathematical physics,—or at least one of such worlds,—is a schematic representation of reality, in which all causes which are not predominating ones are left out of account and treated, for the sake of saving the trouble of attending to many things whose influence is very small, by the calculus of probabilities. Thus, this calculus studies effects about the causes of which we are ignorant; and the usual (Laplace's) definition of "probability" implicitly contains a reference to an assumed non-existence of a predominating cause (cf. S. Pincherle, *Scientia,* Vol. XIX, 1916, pp. 417-426; cf. my account in *Mind,* N. S., Vol. XXVI, 1917, pp. 243-244). It seems, then, that it is when we try to decide, without making use of the notion of cause, which—if any—is the most probable of various configurations at time t' of a physical system that we meet those "terrible difficulties in the notion of probability" spoken of by Mr. Russell (*Ext. World,* p. 36). And, as an attempt to define causality in terms of probability is an attempt to walk in a vicious circle, we can hardly avail ourselves of the permission Mr. Russell went on to give, "—but we may ignore them for the present."

Let us try to make clear to ourselves what sort of questions we have to deal with in the calculus of probabilities. First of all, we may put on one side all assertions about the "probability" or otherwise of a known *proposition* q being implied by a known *proposition* p, the actual deduction not having been made, and confine discussions about "probability" to certain discussions about the truth-value of propositional *functions.* Now, although we may speak, say, of "the probability that $\phi\ (x)$ is true," where we know the form ϕ, the range of x, and the truth-value of $\phi(x)$ for any x in the range (it is m/n, where n is the number of x's in the range and m is the number of those x's for which $\phi(x)$ is true), it is not difficult to see that the characteristic questions arise only when one or more of the arguments are *unknown* coordinates of space or time. Questions in probability are thus essentially questions in physics.

But it is a simple deduction from a known physical law that, of all paths which might be described by a system of known constitution (iron, brass,...) and connections under any forces (gravitational, magnetic,...), that path which is described by the system if it is a purely dynamical one (the given *masses* are relevant and not their constitution, such forces as that of gravitation are alone admitted, and "concealed" masses not assumed) is that determined by the rules of the calculus of probabilities to be the "most probable" one. In fact, Gauss observed at the end of the memoir (1829) in which he announced a new form of the fundamental law determining the motion of any dynamical system whatever: "It is very remarkable that the free motions [of the particles of the system under the given forces], if they cannot take place owing to the conditions of the system, are modified by nature in precisely the same manner in which the calculator compensates, according to the method of least squares, experiences (*Erfahrungen ausgleicht*) which relate to magnitudes connected of necessity with one another. This analogy can be still further followed out, but to do so is not my intention at present" (cf. the reprint of the original in No. 167 of *Ostwald's Klassiker,* p. 30, and my notes on pp. 46-47, 68; cf. also Mach, *M.*, pp. 350-364). This Gaussian "law of least constraint" determines uniquely the motion of the system when the mechanical forces, masses, connection, and initial conditions are known; and it seems that we may define "most probable" as "mechanically determinable." Everybody will at once recall such facts as our greater familiarity with those events which are of a predominantly mechanical nature; the surprise occasioned in us by a physical experiment, such as Oersted's, from which results a motion which cannot, without going beyond the sense-data, be explained mechanically (cf. Mach, *C. of E.*, p. 69); and that the law of the arithmetical mean for averages is a consequence of the law of least squares.

* * *

We will now consider the law of causality as an *a priori* principle.

In all natural science our aim is to complete facts in thought—whether for practical or purely intellectual ends or both. For this purpose we set up a model—a mathematical construction in thought, and so arrange that the logical consequences of premises in our model should represent—approximately at least—the events which have very frequently followed certain other events in nature, while there are other consequences which represent what might be unobserved events. Thus our model might contain Galileo's formula ($\frac{1}{2}gt^2$) for falling bodies, which would give results for times, say in the next century, at which no observations have been made. We then presuppose that it is possible to complete facts in thought; if this supposition were not true, it would obviously be impossible to have any science at all, but only mere descriptions of isolated observations. The case is something like the case of the solution of differential equations in mathematics: solutions of certain more or less isolated kinds of differential equations have been known for over 230 years, but it was less than a hundred years ago that Cauchy showed that, logically speaking, the first question was to prove, in very general cases, the existence of solutions. He did this for many cases. Now, in physics we cannot prove logically the existence of events corresponding to our above-mentioned conclusions, as we could if physics were founded exclusively on logic; and consequently, from a logical point of view, we must *assume a priori* this existence *unless* we find it possible and desirable to deduce propositions from our model without any reason whatever for believing or disbelieving that they represent completions of facts. This last process is analogous to the practice, if it has ever been practised, of setting up functions which may be solutions of differential equations although we have no *a priori* reason for thinking so and no kind of assurance beforehand that solutions exist. It is under the assumption that we must know that it is possible to complete facts in thought that Mr. Russell (*Ext. World,* p. 223) says that "there must necessarily be some *a priori* principle involved in inference from the existence of one thing to that of another"; but he chooses, on the grounds that the formulation of the law of causality is complicated and its assumption *a priori* is therefore unplausible (*ibid.,* pp. 35, 223; cf. *Myst. and Logic,* p. 195), that the "principle of induction" is more fundamental. He rightly remarks (*Ext. World,* p. 36) that Mill's "method of simple enumeration" does not invariably give true results, and therefore discovers a way of saying something involving the method of simple enumeration that is invariably true,—its "probability" increases indefinitely with the number of instances. Apart from the introduction of an indefinable,—"probability,"—in a way that seems to appeal to popular rather than scientific minds, this has the same meaning as: "The propositional function $\phi(x)$ is not true for all x's, but the propositional function 'for every x either $\phi(x)$ or not-$\phi(x)$' is invariably true."

The principle of causality may, it seems to me, be stated without any use of an indefinable "probability" as follows:

There is a one-one correspondence between any portion of the universe and the whole of the universe such that any change in the portion necessarily implies some change of all the rest. Of course it must be added that most of these changes are quite imperceptible to ordinary observation: what is meant is that, speaking with the utmost strictness, nothing in the world can change without everything else also changing. Imagine a person jumping on the surface of the earth: common sense would say at once that it is absurd to say that the jumper moves the whole earth. But if we consider the case of two gravitating masses m and n, of which m is very great and n is very small and in contact with m; and n is alive and jumps a short distance from the surface of m; then the common center of gravity of m and n remains fixed, and consequently the whole mass m moves a very minute distance. Such instances take away the appearance of paradox in the principle of causality just formulated; and we must always remember that, for practical purposes all very minute alterations may be disregarded. Thus, although strictly speaking the correspondence between the variations in any portion and the whole is one-one, for practical purposes we can regard the correspondence as many-one and say that changes may possibly take place without any (perceptible) alteration in most other things. This comes to the same thing as pointing out that these are "practically isolated systems" (cf., e. g., *Myst. and Logic,* pp. 197-198; *Ext. World,* p. 226). In the account given by Mach of the functional dependences of changes in any portion of nature or changes outside, these functions were, since Mach never concerned himself with mathematical refinements, many-one, and consequently he was forced to admit that there are, as there appear to be, cases (of which an example was given in *M.,* p. 503) in which a certain phenomenon B can vary without a corresponding variation of the phenomenon A, although to different A's correspond different B's. It is to be noticed that Mach rejected, as everybody concerned with practical work does reject, the notion of a change determining a change of the *whole* universe.

It is to be remarked that Mach expressed himself very strongly as to the absence of meaning in such a phrase as "the state of the universe." At least he did this in his publication of 1872 (*C. of E.*, pp. 62-63),—but he did not repeat his objections in his *Mechanics*. This may have been due, either to his supposition that the correlation between changes in a portion and in the whole is many-one, or to his definition (*C. of E.*, p. 63; *M.*, p. 502) of "the cause" of A as the *totality* of sense-data which condition the existence of A, and he may have suspected that such a cause may involve the whole universe. But it seems that we might speak of a propositional function involving x, where x is *anything* in the universe, without speaking of the universe as a subject of the propositional function. Further, it seems true that we cannot speak of a space and time which have existences apart from the universe itself; for "space" and "time" seem to be merely convenient ways of describing the relations of the universe to any particular event. But, in our mathematical schemes of the world, we introduce absolute space and time, and, though we might seem to be able to consider the universe in absolute space and time, we find on closer consideration that our schemes are merely approximations to a representation of a portion of the universe which is a practically isolated system. The contrast between the absolute space and time of mathematical physics and the relative space and time of physics will be more fully developed on another occasion; here we are only concerned with the facts that strictly speaking the cause of any effect is the whole universe; and, on the other hand, that it is always possible—often it would seem very simple—to regard for practical purposes physical systems in a small portion of the universe as isolated. It is not asserted that there is an *a priori* principle of causality: all that is asserted is that, *if* there are "laws of nature," they are *a priori*, for it is, in general, logically impossible to determine a law from a finite number of observations,[8] and "probability," even if it could—which would seem doubtful—serve to bridge the gap between observation and law, cannot, without a vicious circle, be used to define "causality," but can itself be defined by means of a certain causal law. Though we may have to assume that there are "laws of nature," we cannot really prove, except by some further hypothesis, that, for instance, the "law of gravitation" is such a "law."

PHILIP E. B. JOURDAIN.

FLEET, HANTS, ENGLAND.

---

[8] If we knew somehow that the law is an *algebraic* function, this would be possible; if it is not, an infinity of observations would always be necessary.

II.

Dr. Charles A. Mercier (*On Causation, with a Chapter on Belief,* London: Samuel George, 52a High Holborn, W.C. 1; price 7s. 6d.; pp. xii, 228) has published a book in which he attempts an investigation into the problems of causality and belief. In the first part of the book, Dr. Mercier deals critically with various theories of causality. In the second part, he expounds his own views concurrently with criticizing Mill's views. In the last part of the book the fascinating subject of the nature of belief is dealt with.

In the course of his criticisms, Dr. Mercier gives short and therefore necessarily inadequate accounts of the views of Hume, Mr. Welton, Prof. Karl Pearson, Mr. Bertrand Russell, and Dr. Mac Taggart. Sympathetic understanding seems to be the basis of all really valuable criticism. It is lack of this sympathetic understanding that makes so much of the criticism in this volume disappointing. Particularly in the treatment of Mr. Russell's theory of cause is this defect plain. The outcome (p. 34) of the critical examination of the theories is interesting and must be quoted:

"In conclusion, it is suggested that the inability of philosophers to define causation in consistent and intelligible terms argues, not that causation is imaginary, but that philosophers are incompetent."

It may be worth while to examine more closely Dr. Mercier's criticism of the theory of causality put forward by Mr. Russell. Mr. Russell has carried his discussion of causality farther than most philosophers, and has stated clearly various difficulties in the traditional formulation of the principle. Thus, for example, in the article "On the Notion of Cause" (*Mysticism and Logic, and Other Essays,* London, 1918), the question of the time-interval between cause and effect is discussed. It is explained that cause and effect cannot be contiguous, for "no two instants are contiguous, since the time-series is compact." Mr. Russell has defined on various occasions what he means by "compactness" (cf. *The Principles of Mathematics,* London, 1903, p. 271, and *Our Knowledge of the External World,* Chicago and London, 1914, p. 132). A series of terms is "compact" when no two terms are consecutive. Contiguous terms are consecutive. Thus it is not correct to say, as Dr. Mercier says (p. 25), "the more compact the time-series, the more closely contiguous must be its instants." It is interesting to see an example here of the kind of alteration the modern philosophical point of view, which is based on logic, has made in the treatment of certain problems. With the technical apparatus of modern logic, it has been found possible to offer an analysis of the nature of such a series as the time-series appears to be. With this analysis, a further discussion of the problems of the time-relation between cause and effect can be given, and the difficulty of allowing that cause and effect are contiguous in time, or are separated by a finite interval in time, has been made clear. For it can only be urged that cause and effect are contiguous in time if it is assumed that the time-series is discrete and therefore discontinuous, and this very few thinkers appear to assume. On the other hand, if there is a finite interval between the cause and the effect, there is always the possibility of something happening during the interval to prevent the expected result. Thus, when I put a penny in the slot, an earthquake may ensue before I can get out my ticket, and thus putting the penny in the slot is not, by itself, adequate to ensure the effect. All this discussion Dr. Mercier sets aside as (p. 26) "having no relation whatever to causation, as it is known on this earth." But people do put pennies in slots, and things might happen to prevent the tickets coming out.

It is to be remarked that Dr. Mercier himself has not seen the difficulty in question, for in talking of a man pushing a trolley (p. 26), he says that "the effect begins as soon or almost as soon as the cause begins." Here is the very difficulty which Mr. Russell is discussing. And thus, Dr. Mercier not only does not dispose of Mr. Russell's contentions by calling them (p. 26) "pretty word-spinning," but actually, on the same page, introduces the identical difficulty without noticing that it *is* a difficulty, and therefore of course without discussing it. Dr. Mercier quotes Mr. Russell's statement of the principle of causality, and says (p. 27) that "by no perversity of ingenuity" could anything which can be discovered by means of it "be twisted or tortured into a cause." But in Mr. Russell's view (*Mysticism and Logic,* p. 180), "the word cause is so inextricably bound up with misleading associations as to make its complete extrusion from the philosophical vocabulary desirable." It is therefore scarcely to be expected that his statement of the principle would give us a way of discovering the cause of anything. Mr. Russell is engaged in formulating a principle which shall be a principle of causality in virtue of its fulfilling the condition of being a principle by means of which "from a sufficient number of events, at a sufficient number of moments, one or more events at one or more moments can be inferred" (*Principles of Math.,* p. 478). In saying (p. 27) that "it is clear that no cause of any thing ever has been discovered or ever can be discovered, for we can never know the state of the whole universe," Dr. Mercier seems to be overlooking Mr. Russell's remark (*Mysticism and Logic,* p. 187) that "it must, of course, be admitted that many fairly dependable regularities of sequence occur in daily life" and (p. 192) "we may admit that, if any such sequence has been observed in a great many cases, and has never been found to fail, there is an inductive probability that it will be found to hold in future cases." This allows the possibility of our being able, with some degree of assurance, to detect casual sequences.

* * *

In Chapter II the constructive part of the work is begun and the relation involved in causation is examined. The following definition is reached (p. 47): "A cause is an action or cessation of action connected with a sequent change or accompanying unchange in the thing acted on." An "unchange" (p. 48) is an effect "which impresses us as....the prevention of change." In Chapter III there is a discussion of condition, and in Chapter IV an examination of (p. 77) "the five characters or marks that are said to be characteristic of causation, viz., equality of cause and effect, immediacy, unconditionality, invariability, and antecedence"; which "shows that not one of them properly or necessarily pertains to causation." The definition of causation is modified and finally stands as (p. 77) "the necessary connection between an action and the sequent change or accompanying unchange in the thing acted on." In the next chapter the doctrine of Plurality of Causes is attacked and Mill's work on it reinterpreted. For the Law of the Uniformity of Nature, Dr. Mercier proposes to substitute an "Axiom of Causality" (p. 101): "Like causes in like conditions produce like effects, and the more closely alike the causes and the conditions, the more closely alike will be the effects." In Chapter VI, twelve methods of ascertaining causes are given. These methods (p. 146), "used by scientific men in scientific matter, are precisely the same as those used by every one else in the common affairs of daily life." Finally a list of the three things which it is necessary to establish in order to prove causation are given (p. 165):

1. Action on the thing on which the effect is produced;

2. Precedence of the action on the change, or accompaniment of the action with the unchange;

3. Connection between the action and the effect, and a list of common "blunders" is added.

Chapter VIII, which deals with the causes of death and the causes of insanity, is absorbing and original and provides interesting examples of Dr. Mercier's methods of inquiry.

* * *

The last part of the book deals with belief. Now, in elaborating a theory of belief, it is essential first to find as many propositions about it which are obviously true as one can. Dr. Mercier does not get to any definite theory of belief, but he points out many interesting things about it which are relevant, and must necessarily be taken into account if a satisfactory theory is to be established. It is, therefore, in this preparatory stage of the work of solving the problem of belief that Dr. Mercier's contribution is most helpful.

It is pointed out that there is an ambiguity in the use of the term belief, that it may mean "being convinced of" or "being sure of"—as, for example, in "I believe there is a God" and "I believe it will keep fine." It is, of course, the latter usage, in which believing and knowing can be contrasted, that it is interesting to analyze, and it is this concept which Dr. Mercier proceeds to discuss. Belief, Dr. Mercier suggests, is an attitude of mind toward a fact or toward a statement about a fact. This statement would be more interesting if the view adopted as to the concept of mind were explained. To say that belief is an attitude toward a fact or toward a statement about a fact seems to mean that an act which is a believing that a is to the left of b is of the form:

x has a certain relation to (a being to the left of b), or

x has a certain relation to S (a being to the left of b) where

S (a being to the left of b) is a statement about (a being to the left of b).

Now there are false beliefs. If this particular belief is false, a will not, in fact, be to the left of b and so there will be no fact which can be described by "a being to the left of b." This difficulty Dr. Mercier clearly feels and it is because of it that an alternative of a relation to a statement about the fact is given. But the interesting point, under these circumstances, is to analyze what can be meant by a statement about a non-existent.

Dr. Mercier next points out the relational character of facts.

He is careful to mention that there are many relations which can subsist between terms, since, he says (p. 192), "the teaching of every book on logic is that there is only one relation between things, and that there is only one verb in any language, namely, the verb 'to be.'" But with this statement as to the teaching of "every book on logic," every student of modern logic is bound to quarrel. The doctrine that there is only one relation which can subsist between terms is certainly (p. 192) "a curious superstition, and well worthy the attention of students of irrational beliefs." The falseness of this doctrine has, of course, been generally recognized by modern logicians, but it is perhaps worth while pointing it out again. But it is a curious mistake on the part of Dr. Mercier to imagine he is making a discovery in pointing out that it is false. The point as to the relational character of facts is again a familiar one to all except those who are entirely ignorant of modern logic,—but the suggestion as it stands is too narrow. It is stated (p. 192) that "our expression of a fact is always in the form 'A is related to B,' and this empty form is filled out and vitalized by substituting appropriate terms for A and B, and by interposing between them a verb as a connecting link, as for instance, 'Hens lay eggs.' This is an expression of a fact, and the fact is expressed by asserting a relation of laying, which means...parentage, between the eggs and the hens." Now we cannot accept this very simple interpretation of the form of the fact "Hens lay eggs," and it seems not extravagant to suppose that there are facts not expressible in the form:

A is related to B.

Thus, in geometry, we have such facts as:

A is between B and C.

Such a fact is relational, but the relation subsists between three terms. It is needless to multiply instances, but modern logic has not at present been shown to be wrong in its theory of the existence of relations between more than two terms. Dr. Mercier's assertions cut out immediately any such theory. By his dictum on p. 217: "Whoso makes an assertion, upon him lies the burden of proof" (if there is anything in such a principle, which seems doubtful), it is for him to prove that all facts are of the dual relational form. Such an assertion most definitely contradicts the views of the modern logician and must be challenged.

In considering various beliefs, Dr. Mercier points out that some statements are not merely false, but definitely nonsensical and such that the mind will not even consider them. Such are, for example, "Two o'clock is solid," "Limestone reasons downward." That beliefs are never nonsensical is an important, if obvious point to be remembered in constructing a theory of belief. The notion of nonsensical as employed by Dr. Mercier is, of course, the same notion as that of "non-significant" which is used in modern logic.

But there are also beliefs which, though not nonsensical, are instantly rejected by the mind. This class are clearly those which contradict the laws of logic. The examples given, "The hen laid an egg larger than itself," "The solid body is liquid," at once contradict definite logical propositions.

Dr. Mercier asserts in the course of his discussion (p. 227) that "belief ought to conform to fact, but cannot be directly related to fact, for we have no direct knowledge of fact. Between belief and fact there is always the intermediary of evidence." Evidence, it appears, is of three kinds—evidence of sense, evidence of reason, evidence of hearsay. Now (p. 193) "when we have an impression on a sense, when we see a light, hear a sound, or feel a touch, these are facts of ultimate certainty; and it is not open to us to doubt that we do experience the sensation." But, in allowing that we cannot doubt that we have certain sensations, is not Dr. Mercier contradicting his previous assertion that we have no direct knowledge of fact? "A fact" (p. 225) means "anything existing or happening in the past, present, or future." Thus the having of a sensation is clearly a fact, and one which we cannot doubt. Further, how will Dr. Mercier treat our knowledge of the truth of logic? Some of our beliefs in logical proposition are not based on anything. Our knowledge of the law of contradiction is, probably, direct. Anyhow our knowledge of some of the propositions of logic is direct.

In his treatment of "empirical certainty," Dr. Mercier makes some assertions which seem to be difficult to justify. He states (p. 227): "That relation which has been found constant....in experiences diverse and incalculably numerous, is true for us." Further (p. 228), "If the relation is not constant in experience, then the degree of belief ought to correspond with the proportion that the positive instances in experience of the relation bear to the negative instances in which the terms of the relation occur apart." But on p. 207 another suggestion is made. We have as before: "If these experiences are sufficiently numerous, and are all one way, we must believe that the experience is necessary, and will continue...." But also: "When experiences are not constant....we are compelled to believe that a similar proportion will hold of such experiences in the future." This is a different proposition from the other. Neither as it stands is satisfactory. They employ words involving all kinds of difficult points (e. g., "sufficiently numerous," "necessary"). The immense difficulties and subtleties of the problem are not even mentioned. It would be almost impossible to deal satisfactorily with all the vital questions which are involved in the small space left to Dr. Mercier for this discussion, but we venture to think that the treatment of the subject suffers from the absence of any hint as to the view taken of probability. The relation of probability to induction seems one of the most baffling problems. Its position is fundamental in any inquiry such as Dr. Mercier's.

* * *

Dr. Mercier's treatise is throughout fresh in its treatment and keen and practical in its point of view. The taunts at philosophers and logicians, though they occur so very many times that one tires of their biting humor and sharp wit, are not essential to the book. One cannot help feeling that a more intimate acquaintance with the work of the modern logicians would cause Dr. Mercier to modify his opinions and change his exceedingly ungracious attitude to philosophers and thinkers generally, which he takes every opportunity of expressing in the present work.

DOROTHY WRINCH.

LONDON, ENGLAND.

III.

Miss Wrinch quotes me as formulating an "Axiom of Causality," a thing I have never done, and should never think of doing, for if causality means something different from causation, I do not know what it means, and if it means the same, I object to calling the same thing by two names, and thus introducing confusion and preparing the way for a fallacy of bivocation.

For the rest, it would be profitless for me to follow Miss Wrinch point by point; for the difference between us is fundamental, and no discussion could diminish it. She regards causation, or causality, as she prefers to call it, from the standpoint of philosophy, as a matter of words, or at the utmost of thoughts, for philosophers to wrangle over. I look upon it from the standpoint of science, as a matter of actual occurrences in the world of experience, occurrences whose nature is to be explained, whose occurrence is to be investigated, identified, and accounted for. I assume that is a thing or event that actually happens in the world of experience. Miss Wrinch follows Professor Pearson and Mr. Bertrand Russell in assuming that it is a word that can be banished from philosophical discussion, and will then cease to exist. There can be no reconciliation between these two attitudes of mind—I apologize for this expression, which Miss Wrinch objects to but which will, I think, be generally understood. If I were to traverse her criticisms, I should be belaboring what seems to me to be shadows, and entering upon a profitless verbal wrangle. I trust Miss Wrinch will not think me discourteous if I say that, living as I do in a world of realities, I have something better to do.

CHAS. MERCIER.

PARKSTONE, ENGLAND.

IV.

I would like to add a little to Dr. Mercier's reply to my criticism. But, first, I must apologize for calling his "Axiom of Causation" an "Axiom of Causality" by mistake.

It is a surprise to me that Dr. Mercier thinks that the difference between his attitude and mine is fundamental. My attitude was merely that of impartial inquiry. When he criticizes the work of great thinkers in a way which seems to me to miss all the important points, it is, I feel, time to lodge a protest. But I am at a loss to understand how this can enable Dr. Mercier to deduce anything about my own opinions—or how they are relevant to the discussion. If my view is of any interest to him—and I could never have guessed that it would be—I may say that the questions which Mr. Russell has discussed in his work on causation do not seem to me to be shadowy, neither do they seem to me to turn on verbal points.

All the criticisms which I have made remain unanswered. In particular it is to be regretted that Dr. Mercier has not seen fit to deal with the points I brought up in connection with modern logical theory, which seems to me to be of vital importance in such subjects as causation and belief. A general and wholesale condemnation of philosophers seems hardly the best way to meet criticism.

DOROTHY WRINCH.

LONDON, ENGLAND.